OUR BOOK SHELF

Experimental Chemistry. Founded on the work of Dr. J. A. Stöckhardt. By C. W. Heaton, F.C.S. (London: Bell and Daldy.)

MANY students of chemistry have had reason to be grateful to Dr. Stöckhardt for his work on the Principles of Chemistry. For many years it was almost the only representative of its class; for it enabled students to acquire a considerable and useful knowledge of chemistry by teaching them to work experimentally at the subject, instead of merely reading about it. One of the great merits of his book, and which also belongs to the volume now under consideration, is that, although the number of experiments described is large and well selected, yet they do not necessarily require anything but extremely simple apparatus. This work, therefore, we believe will be found useful to a numerous and increasing class of students, who, though hindered by limited means and want of opportunity, wish to acquire some knowledge of chemistry.

The work is divided into four parts: the first treating of the General Principles; the second on the non-metallic elements; the third on the metals; and the fourth on Organic Chemistry. With the commencement of Part I. we confessed we were rather startled. The student is at once taken into a sort of half discussion as to whether matter has any existence or not, and the conclusion come to is that the problem will probably be for ever in dispute. This to a young beginner would scarcely impress him with the definite and unchangeable facts of Physical and Chemical Science.

Part I., on General Principles, is, we think, too advanced and complicated for the class of students by whom it is likely to be used. In fact, we should imagine that a student commencing the book and working by himself would find this part very up-hill work. For instance, before having studied any of the properties of the elements, he has to become acquainted with the various methods of fixing the atomic weights, the classification by atomicity, variations of atomicity, isomorphism, &c. In our opinion it would be almost better for a student to commence at the second part, that is, with the study of the non-metallic elements, in doing this, however, some little reference to Part I. would be necessary for the explanation of the meaning of symbols, &c., and he might then return to the complete study of Part I. The first part contains a number of definitions, several of which are not so good as they might be. It is said, for instance, that in a mixture the properties of the different ingredients are always perceptible. Gunpowder is given as an instance of a mixture; but in this the yellow colour of the sulphur and the white colour of the potassic nitrate are certainly not perceptible. Again, the definition of an acid is the following:—"An acid is composed of hydrogen with one of those radicles (p. 86) which are called acid radicles. The hydrogen can be replaced by metals, in which case one of the compounds called salts is formed. Acids redden litmus, and are commonly sour." On referring to p. 86, the exact definition of an acid radicle is not to be found; it is, as nearly as can be expressed, according to the author's ideas, the residue of an acid from which the hydrogen is abstracted. The definition of an acid, then, seems to be a body that contains hydrogen replaceable by metals, which is sometimes sour and reddens litmus. Surely definitions of a rather more definite and complete character might have been selected. Further in the book (p. 298) the author thinks it is often more convenient to regard the inorganic acids as hydrates, that is containing the radicle hydroxyl (HO), and of course uses this radicle throughout the organic acids. If the student accepts the two definitions he will have a double set of radicles, which would probably lead to much confusion.

The second part of the book is devoted to the non-metallic elements, the properties of which are studied by means of simple and instructive experiments, which are generally well described; the same is also the case with the next part, on the metals, and we then pass to the organic section of the work.

The field of Organic Chemistry is now so large that in the small space here devoted to it, a brief description only can be given of some of the more important compounds. It is also difficult in this section to arrange experiments which can easily be performed by students. It is thus necessary to confine the description of such classes of substances as the alcohols, the aldehydes, acids, haloid ethers, &c., to a very few pages. The arrangement, too, is peculiar, the experimental part of the organic work beginning with the study of cellulin, starch, sugar, &c., passing afterwards to the study of the more simple compounds, such as ethylic alcohol, acetic acid, &c.,—which seems rather like reversing the order of things. In a subsequent edition it would, perhaps, be better to adopt the modern system of classification, which would probably give the student a far better and more comprehensive knowledge of the subject. The book is, on the whole, one which, with a little reservation, can be safely recommended to students who wish to study Chemistry in the experimental way rather than simply to cram it up by reading. There is some room for improvement in the woodcuts, which in some instances are not artistic, and might be replaced by engravings of more modern and convenient apparatus.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Ocean Currents

I HAVE just read Mr. Ferrel's letter (NATURE, June 13), in which he refers to mine of April 25, and the proof therein adduced by me to show the physical impossibility of oceanic circulation being the result of differences of specific gravity. Unless Mr. Ferrel means (and I hardly think he does) that six foot-pounds of energy can perform 9,025 foot-pounds of work provided only sufficient time be allowed in which to perform that work, then I do not suppose there is any reader who may have glanced over my article on the subject who will not readily admit that Mr. Ferrel's reasoning has no direct bearing whatever on my argument.

The slope from the equator to latitude 60° is six feet. The total amount of work which gravity can perform upon a pound of water in carrying it down this slope is, of course, six foot-pounds. And this holds equally true whether the pound of water moves down the slope in, say one month, or takes 1,000 years to perform the journey; because the amount of work performed by gravity depends not upon the time which a body takes in descending, but upon the distance through which the body descends. In the present case six feet is the distance, consequently six foot-pounds is the amount of work performed upon the pound of water in its passage down the slope from the equator to lat. 60°.

Mr. Ferrel assumes that the velocity of the general movement of the water advocated by Dr. Carpenter does not exceed one mile per day, and that consequently the resistance to motion must be small. Undoubtedly the slower the motion the less the resistance; but so far as the argument under consideration is concerned, it is a matter of indifference whether we suppose the velocity of the water to be a mile per minute, a mile per day, or only a mile in 1,000,000 years; because it is found that when the water from the equatorial regions reaches, say, lat. 60°, it, as a matter of fact, is not moving eastwards relatively to the earth's surface with a velocity of several hundreds of feet per second, but with a velocity of only a few feet per second, perhaps not more than three feet at the utmost. In this case the water has lost 760 feet per second of velocity which it possessed when it left the equator. Each pound of water has therefore lost 9,025 foot pounds of energy. What has become of all this energy? It has all been consumed in overcoming resistance during the motion of the water from the equator to lat. 60°. But, be it observed, it has

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been consumed, not in overcoming resistance to the poleward motion of the water, but in overcoming the resistance to eastward motion. The energy consumed is deflecting energy, not impelling energy.

According to Dr. Carpenter's theory the pound of water has in virtue of gravity only six foot-pounds of energy to carry it from the equator to lat. 60° against all the resistances to its poleward motion; but it so happens that before the water reaches lat. 60° no less than 9,025 foot-pounds of energy is consumed in overcoming resistance to eastward motion. But if it requires 9,025 foot-pounds of energy to overcome the resistance to eastward motion, how is it possible that Dr. Carpenter's six foot-pounds of energy can overcome the resistance to the poleward motion? The velocity of the motion of the water polewards is as great as, if not greater than, the velocity of the motion eastwards, consequently the resistance to the motion of the water poleward must be as great as the resistance to the motion eastward. But if so, then the work of the resistances to poleward motion is 1,500 times greater than the work of gravity. The work of gravity being only six foot-pounds, whereas the work of the resistances is 9,025 foot-pounds.

One of two things must therefore follow as a necessary consequence: (1) either the work of the resistances to poleward motion is 1,500 times greater than the work of gravity, or (2) the work of the resistances to poleward motion is 1,500 times less than the work of the resistances to eastward motion. But either conclusion is equally fatal to the gravitation theory.

It seems to me that until the advocates of this theory manage to escape from this dilemma, it is needless to argue further on the matter. For, unless it can be shown that the work of the resistances is not greater than the work of gravity, the much disputed question as to whether or not difference of specific gravity can be the cause of a *general* interchange of equatorial and polar water must be regarded as finally settled in the negative.

I cannot help thinking but that Mr. Ferrel is misled by his supposed analogy between a slope produced by the influence of the attraction of the moon and that produced by difference of specific gravity. Although a slope of 9 feet in a quadrant resulting from difference of specific gravity is insufficient to produce motion of the water, nevertheless, the sea will easily regain its level after the attractive force of the moon is withdrawn, even though the height to which the surface of the ocean is raised may not exceed a single inch. The reason of the difference in the two cases must be obvious to any one who will reflect on the matter. I have already in my paper in the Phil. Mag. for Oct. 1871 alluded to this reason, and will have occasion again to refer to it at greater length.

I may notice that by a typographical error in my article the velocity of rotation at lat. 60° was stated to be 773 feet per second instead of 763 feet per second.

Edinburgh, July 18 JAMES CROLL

The Melbourne Telescope

MR. ELLERY has been so good as to send me an enlargement of the lunar photograph taken with the great Melbourne telescope, to which you allude at p. 228, No. 142 of your Journal. This picture, Mr. Ellery tells me, was taken on the second evening of trial; it is very beautiful, although not so *critically* sharp as several I have obtained with my Newtonian equatorial of 13 in. aperture, and a little more than 10 ft. focal length. This sharpness, however, is a mere question of the shadiness of the atmosphere; and I feel persuaded that pictures will be taken with the Melbourne telescope far surpassing any hitherto procured. In my telescope the focal image varies from 1 in. to $1\frac{2}{10}$ in. in diameter, according to the distance of the moon from the earth. The primary picture of the Melbourne telescope (an enlargement of which has been sent to me) is $3\frac{3}{16}$ in. in diameter; hence the structure of the collodion and minute defects in it are of much less importance than when smaller instruments are used.

The employment of the great Melbourne telescope for astronomical photography cannot fail to be of great advantage to astronomy, and I should be very glad to see a similar instrument at work in England, notwithstanding its too much abused climate.

WARREN DE LA RUE

P.S.—As soon as the Melbourne picture has been mounted and protected, I will place it in the Astronomical Society's rooms for inspection.

On the Rigidity of the Earth and the Liquidity of Lavas.

IN his letter upon the Rigidity of the Earth and the Liquidity of Lavas in the number of NATURE for July 11, Dr. Sterry Hunt has replied to my challenge to propose an explanation of the connection between mountain ranges and trains of volcanoes consistent with a rigid globe, other than that to which I refer it, viz. the production of fusion through a diminution of pressure due to the partial support of the mountains by the lateral thrust which has upraised them. Dr. Hunt suggests that liquefaction may take place beneath such ranges, through *increased* pressure promoting the liquefaction of the water-impregnated mass; and quotes the late Archdeacon Pratt as maintaining the existence of a greater pressure beneath mountain ranges.

In reply, I may be allowed to ask Dr. Hunt for a reference to such an expression of Pratt's opinion. I cannot call to mind any passage of his to that effect. The result of his calculations of the attraction of the Himalayas upon the plumb-line showed that they do not attract so much as they ought to do, and he explained this by supposing a deficiency of matter beneath the mountains. His own explanation of the phenomena, as given in the fourth edition of his Theory of the Earth, finished very shortly before his lamented death, is, "that the varieties we see in mountains and plains and ocean beds in the earth's surface, have arisen from the earth having been once a fluid or semifluid mass, and that in solidifying the mass has contracted unequally, so as to form hollows where the contraction has been greatest, into which water flowed and formed seas and oceans, and to leave high table-lands and mountain-ranges where the contraction has been less." (He speaks here of contraction in the vertical direction.) A geologist will, I suppose, receive this as a very incomplete explanation; but the material point is that the Archdeacon was led to adopt it because he had discovered a deficiency of matter beneath the Himalayas. This seems incompatible with Dr. Hunt's view (both with regard to Pratt's opinion on the subject, and with regard to the fact itself) that there is an increased pressure beneath mountain ranges.

It will now appear that my "speculation" upon the origin of volcanic action was suggested by the proved deficiency of matter, and consequently probable diminution of pressure; and not that the idea of diminished pressure was invented to account for volcanic action. I have merely proposed a connection between lateral pressure and diminished density which seems most natural, namely, that the same pressure which upraised the mountains continues partially to support them. And I cannot see how it can do otherwise. For the abutments of the mountains having approached by contraction of the crust, cannot again recede without expansion, which cannot take place. Dr. Hunt's view of the liquefaction of lavas, to my mind, requires explanation. Admitting that pressure promotes aqueous liquefaction in heated rocks, when rocks so liquefied began to rise in a volcanic mass, would they not be brought under diminished pressure, and would they not become immediately solidified, so that they could not reach the surface in a fluid state?

Moreover, since liquefaction, according to this view, is increased by pressure, the interior parts of the earth being under greater pressure than the more superficial strata, ought, at least to that depth where water is present, to be more liquid, and this would be incompatible with the supposition of a rigid globe which Dr. Hunt favours.

Harlton, Cambridge O. FISHER

The Method of Least Squares

WILL you allow me to call the attention of Mr. J. W. L. Glaisher to the following sentence from Encke, Berliner Jahrbuch 1853, p. 311. "Ich werde mir deshalb erlauben, völlig dem Gange den Lagrange genommen hat folgend, wie könnte man sich erdreisten, bei der ungemeinen Klarheit, Einfachheit und Tiefe des grossen Meisters, eine irgend bedeutende Änderung vorzunehmen, den Theil der Abhandlung hier wiederzugeben, welcher den Beweis für das arithmetische Mittel enthält, und selbst Sätze, die im Grunde schon die Methode der kleinsten Quadrate in sich begreifen." Also to article 17, Corollary, of the Memoir of Lagrange.

This is not the place to discuss the doctrine of the Method of Least Squares; but I may say that in my judgment the method rests on the assumption of the principle of the arithmetical mean, an assumption which is justified by an universal experience.

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